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Interactive comment on "Beneath the arctic greening: Will soils lose or gain carbon or perhaps a little of both?" *by* Jennifer W. Harden et al.

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Response to Anonymous Referee #1 General Comments

1. I have a strong major concern with the layout of the study. Using a space for time approach the authors compare three single soil pits with thousands of miles distance in between. The authors take the data of 3 soil pits and model soil OC development over 300 years into the future. All uncertainties, all vegetation and climate and parent material differences are just neglected, and the whole model is based on some 14C and C data. The results look feasible (of course there will be depth trends in OC), and they might be if you think a Gelisol might become an Inceptisol and Mollisol with changing from a 300 mm to 800 mm precipitation ecosystem. But the whole study

overstretches the space for time approach by far! It is already complicated to correlate soils in one catchment using this approach, but on completely different parent materials and ecosystems...

We appreciate Referee #1's concerns about our application of the space-for-time substitution approach to this study. However we disagree with the assertion that state factors are "are just neglected." As we state in the manuscript and now clarify further, we aimed to control for parent material, age, and slope across the study sites. We have revised the text in section 2.1 as follows:

"Soil samples were collected and analyzed from three study sites including a Gelisol in interior Alaska (n = 4 profiles), an Inceptisol in south-central Alaska (n = 14), and a Mollisol in Iowa (n = 3). Parent material, time (age), and topography are three of the primary soil-forming factors known to influence soil properties (Jenny 1941) and soils for this study were similarly underlain by late Pleistocene loess (silty sediment of wind-blown origin) as a common age and type of parent material and slopes of 3-10% as a common topographic setting. Other soil forming factors (climate, vegetation) varied across sites, providing a means to test the effects of changing climate and ecosystem state on soil C storage and flux. For example, Iowa Mollisols formed with little or no permafrost throughout most of the depositional history of loess accumulation and organic matter burial. Alaska Inceptisols formed with no permafrost since at least 5,000 y BP as evidenced by a 5 ka volcanic tephra at $\sim$1 m depth that was not deformed by frost heave; and Alaska Gelisols formed with continuous permafrost since the Pleistocene."

We now have included information on soil texture and particle size in the Methods section, and have provided a link to the data at the International Soil Radiocarbon Database. These data further support our consideration of parent materials across the chronoequence. Also see section 2.1 for each Gelisol, Inceptisol and Mollisol. (clay content ranges within and between sites from about 5 to 20% clay).

As described above, we accounted for relief across the chronosequence, which is an-

other state factor described by Jenny (1941). All three soil profiles were sampled on hillslopes of 1-10% slope. We added text to the Methods to better describe topographic position of sampling sites.

We acknowledge and clarify in the Methods that vegetation and climate have varied across the sequence. Figure 2 (formerly Figure 1) shows the close relationship between climate (i.e., soil temperature) and time. More specifically, present-day soil temperatures across the sequence closely track projected changes in soil temperature for a permafrost site out to 2100 and 2300. To be clear this study offers a constraining conceptual model in which climate, ecosystem and soil state are represented by current day steady-state systems. We tried to not overstate this conceptual model but feel strongly that the science community could use current climate-ecosystem-soil associations to constrain (in particular) dynamic vegetation models. We have added and clarified text throughout the manuscript to better support our approach, identify our assumptions, and highlight possible limitations. For example on page 4, we refer to our “space-for-time substitution approach” and that “We modeled the gradient as a warming scenario in order to conceptualize how climate, ecosystem shift, and soil state might transition from their steady state”

2. The warming Arctic and its OC fate is a big topic, but is this worth putting together old data and squeezing it into a questionable modelling approach? You have a nice data set, so maybe its worth rethinking your approach and re-write it with what it is, three single soil pits. Based on that you could really go into detail discussing OM distribution and possibly also stabilization, but not telling a story that "this" Gelisol might be "this" Mollisol in 300 years.

We choose to maintain this exercise for the following reasons. First, a primary objective of the manuscript was to illustrate one possible application of the data through a very simple modeling framework. Through this simple modeling approach, we were able to highlight dynamics of different soil fractions given variable 14C-based turnover estimates in response to ecosystem changes. In many modeling studies, turnover rates

for different soil pools are derived empirically from incubation of bulk soils or ecosystem fluxes, not from observations of different pools. Thus, our study represents a novel approach, including both observational and modeling approaches for specific soil C fractions.

Second, another primary goal of our study was to provide some constraints on possible C changes following thawing of ice-rich Pleistocene loess. This is a globally important C pool, and the fate of this C is poorly constrained both by observations and models, particularly at the decadal and century time scale. Other data-driven modeling approaches have used relatively short-term incubation data (month to annual time scale) to drive decomposition rates to estimate the permafrost-carbon feedback. Our radiocarbon-based technique is a more appropriate approach for constraining C dynamics over longer time scales.

Third, while ecosystem transitions are commonly modeled based on climate shifts (both for back-casting and forecasting) the link between aboveground (vegetation) and belowground (soils) generally are based on biogeochemistry models and not on ecosystem – soil associations. This rather deliberate approach to ecosystem-soil opens up entirely new data from soil surveys associated with land cover and land use change that we think can strengthen our understanding of these linkages.

Last, outcomes of the modeling work should not be interpreted literally (i.e., a Gelisol might become a Mollisol in 300 years). While our simple model is based on observations, results should be interpreted with caution, given the limitations of our approach. The goal of the model was not necessarily to be predictive, but to better understand dynamics associated with ecosystem change. We also added a new conceptual diagram (Figure 1) at the request of the Topical Editor.

Specific Comments 1. page 2 line 31 and following: If you only look into Hugelius this might be right for the permafrost regions, but there is a growing number of studies on subsoils globally. With this there is also a growing understanding of what drives subsoil

C stabilization. There is also already some work on SOM fractions in the Arctic, so maybe worth checking for OM vulnerability in permafrost soils (e.g. Gentsch et al. EJSS 2015; Mueller et al GCB 2015).

We added text here to clarify that we are specifically referring to soils “in the northern permafrost region.” We also added as sentence to reference to work of Mueller et al. (2015).

2. page 3 line 25-29 I doubt that todays arctic permafrost soils can via a space for time approach be related with Iowa soils. Space for time approaches even when conducted in the exact same ecosystem have a tremendous number of assumptions. In your case you are pushing these assumptions far of a meaningful level. page 3 line 30 and following - I clearly doubt that the research sites can give you a reliable answer. Of course you see differences between the sites, but what are the factors driving these changes, definitely not just a permafrost you find at one spot but not the other!

Without overstating our “conceptual experiment”, if there indeed are new grass-dominated ecosystems in a drastically warmer arctic, it is conceivable that Mollisols will form underneath them and that their roots will colonize deep, unfrozen substrate. After all, there are some Mollisols in Alaska today. Moreover, we emphasize that the important differentiation among the sites is the association of climate-ecosystem-soil and that indeed the soil carbon dynamics in each site are indeed indicative of carbon storage and turnover for those associations. On this point we will have to disagree. Please see how we handled your objection by reviewing the discussion section, in particular on pages 15-16: “. . .these comparisons illuminate the potential for these (physical, chemical, biological) mechanisms to shift under changing climatic and ecosystem states.” Also, “Several important caveats should be noted in this approach”, and “Our approach is a comparison e.g. a space-for-time/climate/ecosystem shift and is not literally a process-based model but rather is an exercise to more deeply understand the how and why soil carbon might be stabilized or destabilized in differing climate and ecosystem states.

2. page 4 line 8-10 Please give detailed mineralogy together with pedogenic oxides to show comparability of study sites with respect to aggregation and organo-mineral associations.

Citations within the manuscript provide detailed field and laboratory data, as do the databases in which those data now reside (International Soil Radiocarbon Database)

3. page 4 line 14 - You get Tephra in at 1 m, so how are you dealing with different nutrient contents?

We have no explicit form of nutrient associations with our carbon data.

4. page 5 line 4 - Actually approx. 3000 miles in between sites.

Yes, this is true.

5. page 7 line 8-17 On top of the differences you also compare soils with and without carbonates? Even under comparable climate you0 ll have differences in OC storage/stability due to carbonates vs. no-carbonates.

All of our data are carbonate free and indeed there was no evidence for carbonate in these soils.

6. page 7 line 18 - How representative were these single soil pits for the area (bulk density, mineralogy, C/N etc.), and thus how representative to relate these soil types?

While other data exist and we encourage more elaborate modeling with regard to new ecosystem-soil associations in the future. This paper is not tackling spatial variation across soil profiles and sites.

7. page 7 line 18-22 What density agent was used? Please briefly describe the procedure.

We added text to note that “sodium polytungstate” was the density solution, and we describe the procedure in depth on Page 7.

8. page 7 line 21 - What is your "occluded fraction", a light fraction or a mixed particulate together with minerals fraction?

We added extensive text on page 7 to clarify how each fraction was determined and defined, including the “occluded” fraction.

9. page 8 line 2-5 - On what was the OC input based, field data, assumptions? What are the input rates of the fractions? Were differences in OM chemistry of the input taken into account?

Input is not explicitly modeled in this paper.

10. page 8 line 7-4 You are taking a modelling approach from a study that models physical OC transport at profile and landscape scale, to model depth functions of OC stability/mean residence time. The assumptions are based on soils from Iowa, but taken to the continuous permafrost Arctic. How are permafrost table depth, root input etc. related to your model assumptions?

This is not a dynamic model, rather it is a calculation from one steady-state system to a new steady-state system. The simplicity of this approach is its strength – if today’s ecosystem-soil association is taken as a hypothetical proxy and if we know something about tomorrow’s distribution of ecosystems, then we simply calculate how the soil carbon might change along with the ecosystem. This transient response along the way to the new state – such as changes in permafrost table, vegetation are not specified. We’ve added text to the manuscript to clarify this point in numerous spots.

11. page 8 lines 15-20 - You are leaving out the unique features of permafrost soils by neglecting the vast amount of OC stored at depth. This also completely neglects soil erosion and changes in hydrologic conditions with permafrost thaw, which are well known to tremendously affect OC storage/fate/turnover. But those would be the step in between your studied sites.

Correct. While there are many unique processes specific to thaw in permafrost ter-

rain, we are simply considering transition from one steady-state ecosystem to the next, without accounting for a specific mechanism of change (e.g., erosion).

12. page 9 line 1-2 - This assumption is so far off! There are tremendous degrees of uncertainty already for concepts like "storage potential" but definitely for the fate of OC in permafrost soils. You are modelling your data to 200 cm depth, and obviously hit a cryoturbated pocket in the Gelisol in the 14C data. The other soils were sampled much shallower but you assume something underneath, which is definitely highly speculative especially given the sight underlain by Tephra. With your approach you could also go a step further and include hot aride loess soils in central China.

We have added text throughout the manuscript to better describe the assumptions and limitations of our approach.

13. page 11 line 8-14 This is all based on assumptions! You did not measure a single k for any of the fractions. This might be vaque for one site, but for a comparison and especially as a base for forward modelling, this is far off!

Decomposition coefficients (i.e., k values) are based on turnover time constrained by radiocarbon in each fraction using a steady-state modeling approach (e.g., Trumbore 1993).

14. page 13 line 12 following - The whole paragraph is only based on assumptions! Where is rooting patterns and biomass data? Were is mineralogy/hydrology data? While the 14C data is based on soil horizons as well as depth layers, which kills comparability already. Okay, not to mention you bring up a story on sites thousands of miles away from each other...

Rooting data are in Table S1 as field descriptions of abundance and size. There are no biomass data, and mineralogy data are in citations. Our 14C data are for depth and fraction – we are uncertain what the reviewer is referring in their critique. See above for revised text.

15. page 14 line 9-10 - You don't have any loss between the two. The only thing you have is three sites with different OC stocks, distribution and composition and you model this data. So you could maybe "assume" differences in these measures between the analysed soils, but to relate them on a timescale of 300 years - this is not based on data! page 14 line 28 and following - This is all right, you demonstrated differences in the distribution of free vs. occluded POM and mineral-associated OM. But you can not draw a line between these distant soil types with respect to one develops from another.

We changed wording to "comparison and postulated transition. . .alludes to losses". We have taken steps throughout the manuscript to remind readers that this is a comparison set up as a conceptual "space-for-time" model.

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